NOTE
DR. MORGAN ON THE MEASUREMENT OF ATTENTION

By K. M. DALLENBACH

In the introduction to his work on "The Overcoming of Distraction and Other Resistances" (*Archives of Psychology*, no. 35, 1916) Dr. J. J. B. Morgan offers some criticisms of my paper on the measurement of attention (this JOURNAL, 24, 1913, 465 ff.), which call for a reply.

(1) The principal criticism is as follows (p. 8). "Because this correlation [between clearness-scale and speed and accuracy of work] was rather high the conclusion was drawn that the true measure of attention is a subjective scale of clearness. Such a conclusion presupposes that the amount and precision of the work was influenced only by changes in attention. This could not be known unless all practice, fatigue and feeling factors were eliminated. But supposing that the experimenters had eliminated all these (which they certainly did not), why not take the amount and precision of the work as a measure of attention directly and not by a roundabout procedure, involving endless difficulties, adopt a measure which is from the very nature of it much less precise?"

A reader of these sentences would hardly guess that the 'presupposition' referred to is practically a quotation from my own paper. The conclusion which I draw from my experimental results holds, I say, provided that certain things are true; and one of these things is "that the work itself is not influenced by anything other than a change in the attention" (p. 499). This, my critic affirms, cannot be known unless practice, fatigue and feeling are eliminated. The suggestion again comes from my paper. I sought to stabilise practice by a three months' drill (p. 472). I sought to prevent fatigue by frequent rests and a division of the experiment (p. 474); the duration of the single observation was 30 to 60 seconds (pp. 474, 493 f.); and only five or six of these observations were made in the hour (pp. 474, 492). I sought to control feeling by requiring a report of affective mood (p. 492) from observers specially trained in introspection (p. 467). If dogmatism were at all permissible in science, it would be nearer the truth to say that the experimenters *did* eliminate these sources of error than to say, as Dr. Morgan roundly declares, that they did *not*. I should be unwilling to dogmatise on either side; but I point out that Dr. Morgan's criticism does not bear upon omissions of my work, but simply denies that I have succeeded in doing what I very carefully tried to do. What his grounds for denial are, I do not know.

Nor do I know how a method of direct, face-to-face observation can be termed 'roundabout.' To measure attention by something else, that is not attention, would be roundabout; to lay a scale of observational measurement upon the attentive state itself is, I should suppose, the most direct course that an experimenter could follow.

But I could not, in any case, measure attention by "amount and precision of the work." Dr. Morgan will hardly maintain in serious-

ness that amount and precision of work are, in general, influenced by nothing else than attention. And even if that procedure were legitimate, my problem was to measure attention in terms of clearness.

(2) Yes, rejoins Dr. Morgan, in terms of a theory! "Experimental work instead of seeking to sift down facts has been used to support one theory or another" (pp. 1 f.). I do not see how an experimental problem can even be formulated without some sort of preceding theory or hypothesis. But I can assure Dr. Morgan that my results would have been published if they had negated the hypothesis which they were meant to test. I think that I have brought out facts of observation, and I think that they confirm the hypothesis in question. If Dr. Morgan can show that they support any other and contradictory hypothesis, I shall endeavor to test that.

(3) A minor point of criticism concerns Wundt's rule for the choice of distractors within the sense-department. Dr. Morgan asserts that I distracted from the tone of a variator by "sounds varying from a metronome to a graphophone" (pp. 4, 8 f.). Would the reader gather from this account that I used eight sets of distractors, including flicker (9 rhythms and 4 intensities) and the electrical current (3 intensities)? Or that phonograph and metronome were the *only* sound-distractors that I employed? Or that I have stated the order of effectiveness of the distractors, in the light both of my objective results and of the observers' reports (pp. 472, 488)? If Dr. Morgan really thinks that the use of the phonograph brought the sound of the variator anywhere near the differential limen (which is the gist of Wundt's rule), my own and my observers' experience proves that he is mistaken.

(4) Dr. Morgan makes me conclude that "since the only way to measure clearness is introspectively, therefore introspection must be the only measure of attention" (p. 2). I nowhere draw that conclusion. The dogmatism is, again, Dr. Morgan's and not mine.